The Moment of Truth: Probability Theory and Standards of Proof

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THE MOMENT OF TRUTH: PROBABILITY THEORY
AND STANDARDS OF PROOF

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Specialists in all the fields involved agree that the process of proof and persuasion in judicial proceedings presents problems in the application of probability theory and communication theory.¹ When the broader term “information” is used, the problems coalesce, both in judicial trials and in other human affairs.² In jury trials, one focus of this coalescence is the formulation for the jury of issues of fact to be decided by them, and their progress to verdict or disagreement.

Although Cicero asserted that probability is the very guide of life, and Thomas Jefferson thought mathematical reasoning and deductions were a fine preparation for investigating the abstruse speculations of the law, it is often asserted that the complexity of judicial fact determinations prevents the application of any probability theory to them, except in the vaguest and most unspecific terms.³ The contention that no rigorous assumptions or rules are justified would seem to place on the contender an obligation to make none in his own operations. It is the purpose of this article to show by means of illustration that (1) some highly restrictive axioms have (consciously or unconsciously) been incorporated into the standards of proof for factual issues and the directions for decision; and (2) that these axioms are internally inconsistent, as well as in disagreement with views held in common by leading exponents of probability theory and application.

One area in which a comparison of legal rules with probability theory may be helpful is that of the burden of proof, or measure of persuasion. It is a truism to say that among the material propositions of fact asserted in a lawsuit by one party or the other, many or most rest upon empirical generalizations which cannot be shown to be logically true, i.e., true for all logical possibilities. At most they, and the propositions, can be highly probable. The assertion by one

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3. As a slightly hedging example, see Larson v. Jo Ann Cab Corp., 209 F.2d 929 (2d Cir. 1954). There is dissent: Loevenger, Jurimetrics, 33 Minn. L. Rev. 405 (1949).
party that X occurred; and by the other that X did not, is a convention which enables the court to test the logical relations of the parties’ assertions at a preliminary stage, as well as the correctness of their implicit assertions concerning the rules of law involved. By the time two contradictory material propositions reach the stage of determination, some required degree of probability is all that can be assigned to them. When the trier of the fact has performed that function, another legal convention, which is supposed to distinguish judicial proof from proof in general, takes control. The trier’s finding of the required probability is treated, for the purposes of entering a judgment and spelling out the rights of the parties then and thereafter, as if the propositions found were true. In Wharton’s phrase, the probable proposition is converted into “juridical truth,” for the purpose of settling the dispute with finality and permitting the other affairs of the litigants and the world to go forward.

If we ask, what is the required probability, we are confronted with a mass of definitions which reflect conflicts, not only over whether any probability theory applies, but over the nature of probability itself. Since the close of the eighteenth century, common law courts have been agreed that in criminal cases, guilt must be demonstrated beyond a reasonable doubt before a verdict for the state may be returned. In civil cases, putting aside certain special types, the finding must be based upon “a preponderance of the evidence.” At this point the courts divide. One group treats this latter term as meaning any preponderance, while others require that the preponderance be a “fair” one, or that the jury “believe in the truth” of the fact, or be “satisfied” or “convinced.” Wigmore has commented caustically upon the communication muddle which has resulted from attempts to define all these terms. One point that has caught the attention of all writers is that the criminal requirement and the second group of civil definitions seem to refer expressly to the state of the trier’s mind, or at least the “reasonable” mind, while the term “preponderance of the evidence” does not in terms do so.

This apparent conflict reflects one which has raged for centuries in probability theory itself. It is sometimes thought to be, and most writers in the legal field have treated it as, a conflict over whether probability is “objective” or “subjective.” If we insist upon an objective universe in which “the facts” partake of logical truth, probability can scarcely be an intrinsic feature of the “facts” or the “evidence” of them. When we say with Aristotle that the funda-

5. 9 WIGMORE, EVIDENCE §§ 2497-98 (3d ed. 1940). The cases are classified in annotations in 147 A.L.R. 380 (1943); 93 A.L.R. 155 (1934); 67 A.L.R. 1372 (1930).
mental property of any statement is that it is either true or false (and cannot be both true and false), we may project this onto Karl Pearson's "world beyond our nerve endings," and say that a fact either is or is not—it cannot partly be and partly not be. We may hold, with Bernoulli and La Place, that everything out there is determinate; but that our knowledge of it is inadequate, and that the "probability" of an event is an expression of the extent of our ignorance. The tendency is then to cast probability theory into "degrees of belief." There is no question that a jury bases its action upon introspection—this is its "deliberation upon its verdict." Writing from this point of view, Morgan,6 McBaine,7 and others have brilliantly pointed up (1) the practical impossibility of requiring the jury to find the determinate "truth," and (2) the straddling involved in adopting some standards which point toward the jury's mental state and others which treat probability as an intrinsic quality of the "evidence" and therefore of the "facts" themselves. They have attempted to come down on the epistemic or knowledge-versus-ignorance side, by urging that the jury find whether it is "almost certain," (or "very highly probable") or "more probable than not" that a fact existed.

The difficulty is that the directions they propose, although this feature is not stressed, come at a point where they are always preceded by words such as "if you find from the evidence that."8 This opens the door again for the adherents of findings of "true" fact to put examples which suggest that a "mere" preponderance of evidence or of "chances" as a standard is inadequate and unfair, when the truth is there for the finding; or that when a matter is of as serious and important a nature as a law suit, one should not act on a slender margin.

With a view to returning to this question for closer analysis, let us first consider a very simplified précis of one theory of probability, which has had the most systematic use and the most verified success in application. To a very considerable extent, although not entirely, it is able to avoid most of the philosophical and logical questions which beset other probability theories.9 This does not mean that it has no assumptions and no problems—by "avoid" is meant "keep away from," in going toward its goal. If the goal is reached, we may or may not wish to say that the difficulties have been rendered immaterial in practice. The greater part of the operations and

8. Id. at 262; Morgan, op. cit. supra note 6, at 84.
9. Good § 1.4 contains a summary of some of the systems.
results given here would not, I think, be materially changed by choosing another theory. It seems particularly adapted for use in analyzing our problem, because it is explicitly or implicitly used in the discussions of relevancy by legal writers, and is explicitly the basis of a great part of the “scientific” evidence received and acted upon in judicial trials. Whether the refusal to carry it fully into the weighing of evidence is due to misunderstanding or is a vestigial remnant of trial by ordeal and by battle (in which the decision was true because rendered by supernatural forces) is a question for the historian. In addition, some such view as the one set forth is thought by many exponents of probability theory to offer the best bridge between abstract probability and empirical reality.

The explanation of probability here given is a form of frequency theory. It is concerned with the numbers (or amounts or proportions) of events of a specified set having various attributes. A simple illustration consists of a sample of 10 apparently identical coins, 8 of which have the attribute “genuine.” Then 8/10 may be called the sample ratio, or proportion of the attribute; it is the proportion of genuine coins. If we attend to a single one of the coins at random, without any way of knowing whether it is genuine or not, we say that the probability that it is genuine (given that it is one of the coins and that the proportion of genuine is 8/10) is 8/10. But all that we have really expressed, and all that we really have to go on, is the original knowledge. Nothing in it tells us whether “in truth” or “in fact” this is a genuine coin or not. A scientist who wanted to be more precisely informed, would say: “You have told me that your total information consists of knowledge that there are 10 coins, 8 genuine, and that if, one by one you attend to all the coins, 8 will be genuine and the rest not. As to whether this coin is genuine or not, your data is simply non-informative—it tells you something about the classes, but nothing about any particular member of any of the three classes. When you say that the probability that this is a genuine coin is 8/10, you have said no more than when you said that the number of these 10 objects which have the attribute is 8. Had you known, now, that all were genuine, or that none were, I should agree that you have information about whether this is a genuine coin or not.” The answer, of course, is that if we knew about every one we would know about every one, and probability, which is uncertainty, would be out of the picture. If the scientist sorted out the counterfeits so as to make it seem almost certain to him that he knew, and we asked him, “Is this coin genuine?” his answer of “yes” would tell no more than his original “Now, all of these coins are genuine.” To quote Hans Reichenbach:
STANDARDS OF PROOF

That some happening is 60-percent probable means that it happens in 60-percent of all cases—any alleged surplus meaning of probability is unverifiable and empty. . . . Statistical laws are not “less dignified” than causal laws—they are more general forms, among which the causal law represents the special form of statistical correlations holding for 100 percent of the cases.\(^\text{10}\)

And, if we speak strictly, causal laws (certainty) never hold in observational terms if the measurements are fine enough. They hold within narrow limits in a very high proportion of observations, and we treat the deviations as “errors of observation,” in order to retain simplicity, and in the effort to join the laws up to a determinate world of facts which “truly” exist.

All our empirical generalizations are, at bottom, of this kind. A friend engages in pistol practice, and the scorer makes a record of his hits and misses. I am not able to perceive them, nor am I told about each result. From time to time, the scorer tells me my friend’s cumulative percentage of hits. After about a hundred rounds, he announces 80 per cent hits, and asks me the probability of hits in the next ten shots. If I have not attended to the fluctuations in percentage, or draw no indication of change from them, I shall argue from the “sample” to another part of the indefinite “population” of shots, and say 80 per cent. Naturally, there is nothing logically imperative about this estimate. If the percentage has been recently rising or falling, I may see a “trend,” extrapolate it by thinking it will go on as it has done, and add or subtract something from my 80 per cent. If a breeze has sprung up, I may consider whether my friend understands windage, and adjust the estimate for that. In that case, I am modifying the basic knowledge by what I think I know about winds and pistols and my friend. I am narrowing my reference class—the scientist would say “adding relevant parameters.” In the end, I have a class of classes, and an estimate. Of course I do not mean that the next ten shots will each be partly in the black—the count is on an all-or-nothing basis. I mean, if I see no basis for adjustments, that my best estimate is that of the next 10 shots, 8 will be hits. Will I do otherwise if the matter becomes “important,” or of a “serious nature,” as it might if I were required to bet $100 on each shot? I should bet on a hit every time. If the bet were changed to a single one of $1,000 on the next shot only, I should do the same, notwithstanding my realization that “for individuals there are no statistics, and for statistics no individuals.” And so, I am sure, would Richard

von Mises, who coined the phrase. I should merely think of this individual hazard in such a way as to bring it within a set of hazards which I believe will come out most advantageously if I use what I know in the way men have always used what experience teaches.

To quote Reichenbach again:

In this way, the frequency interpretation of probability can be applied to a single case; we use here a transfer of meaning, a logical device which can be justified through its expediency for the purpose of action.

It is not necessary for this transfer of meaning that there always be long series of similar events. Events of very different kinds and of different degrees of probability can be regarded as forming a sequence. The series of actions within the life of a person is of this kind. If a man makes it his rule always to expect the more probable event, he will be right in the larger number of cases.11

Now let us consider a past event. The scorer asks, what is the probability that the rounds 80-89, of which the record is here, were hits? Will I act otherwise than in the first case, on the theory that future events are uncertain and we must use such feeble methods as extrapolation from our experience of frequency, whereas in the second case there is a true, determinate, past set of facts? I shall give the same estimate of probability; if required to bet 10 times, I shall bet on 10 hits, and the same for round 89 alone, if that is the one thing which I am forced to make an assertion about. All the while, my data told me nothing about whether round 89 was a hit or not, nor about whether round 101 will be a hit or not. Likewise, if I were forced to take these hazards, I should not change my way of proceeding even if the sample showed only 52 per cent hits, or 51. It is unreasonable, I think, to say that the probability exists, or is useful, only when it is high, if the value of a correct decision equals the cost of an erroneous one. The reader who thinks such data should be ignored in important matters, should consider an example based on one by Gibbon. Given his choice of one of two lotteries to select one victim each for public hanging, would he enter the lottery in which 51 tickets are being issued, or the one with 49?12

If we examine the courts' handling of problems of relevancy, we find that a form of frequency theory is continually applied to determine whether a particular piece of information changes the probability of a material proposition. Wigmore13 has shown that the opinions usually speak an inductive language, which only implies the existence of a frequency distribution relating to the matter under consideration; but there is no question that behind every relevancy

statement is a proposition which casts human experience into frequency form. To say that the existence of motive for murder tends to show the commission of that crime by the possessor of the motive, we must appeal to the generalization that more murderers have been found among men who have motive than among those who lack it. To say that a need for money helps to prove the making of a loan, we must say that money is more often borrowed by those who need money than by those who do not. When we say that the posting of a letter, properly marked and stamped, raises a presumption that it reached its addressee, we appeal to the proposition that the mail goes through in a high proportion of cases. None of this data, as we have seen really tells us anything about the specific case. We know that most of the men who have motive to murder do not commit it; that not everyone who needs money borrows it; that some mail goes astray. There is nothing in our statements, even if they be made numerically precise and correct to the last item, which tells whether the case at hand is one or the other of the two kinds covered. None the less, lacking more specific information, we shall make the fewest mistakes if we treat those statistics as representing the probability that the case is one or the other and acts accordingly.

The determination of the states of probabilities by weighing evidence is no more than an extension of the same determination which is made for rulings on relevancy. Whether a piece of evidence or other information tends to prove a material proposition is treated as an all or none matter. It either changes the probability or it does not. Efforts to raise a standard of "legal," as distinct from logical, relevancy confuse the value of the chance with its existence. When the time comes for a finding to be made, we concern ourselves with the size of the probability. In setting a requirement of persuasion, we also look at its value. But the process, and the use of estimates based on experienced frequencies and frequencies learned from the evidence, does not alter in any other way. We deal, at the moment of decision, with probative force as a quantity relative to other quantities, rather than merely asking whether it is present or absent. This task may be more difficult, but it is different only in degree. We examine more closely the frequency distributions involved, and perform operations on them to create new classes with different distributions, in an effort to bring our estimates within narrower limits; and the most "conclusive" proof can accomplish no more than

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14. James, Relevancy, Probability and the Law, 29 Calif. L. Rev. 689 (1941), always cited in relevancy discussions, effectively refutes Wigmore's idea that the inductive form is just as useful. Its only use, in analysis, is to obscure the major premise.

15. Michael & Adler, The Nature of Judicial Proof 99 (1931) discusses the point, although not from a frequency point of view.
this. The fingerprint expert, with millions of distributions accurately
catalogued, creates for the finger tip of the defendant a class of classes
which, extrapolated for the universe of fingerprints, is so rare that
we should expect to examine more than 64 billion men before finding
a second example. With the earth’s population between two and
three billion, he asserts that the planet holds no other, and that it is
“mathematically certain” that the fingerprint at the crime is also
defendant’s. Most of us will accept this as meaning that even though
the two in 128 billion could occur in the first few billion, the odds
against this being so are sufficient for action. But there is nothing in
the data which tells us that the “truth” is that the case at bar is one
of the 128 billion and not the two. What we do draw forth is the
number of mistakes we shall make by each course of action. And our
willingness to choose the course of two mistakes instead of billions
seems strangely inconsistent with the stubborn insistence sometimes
found, that if 49 mistakes in 100 cannot be avoided, we should forget
the data and try to make at least 50, not because the consequences
of the mistakes differ, but because we construe this creation of
error as a safeguard for the concept of truth.

This sea change is not so noticeable in circumstantial evidence
cases, and is almost completely missing when the future is involved.
The use of probability estimates regarding future events and
conditions has been justified numberless times by assertions along
the lines of “The future is uncertain, but man must act.” To the
extent that courts deal, and allow juries to deal, in predictions about
future events, they show no hesitation in the application of proba-
bility estimates based on frequency statistics, nor is any great rigor
exerted in ruling upon the substantial similarity of the conditions
and assumptions involved in the estimates to the immediate case at
bar. By this I mean that the courts (and legislatures) do not insist
that the case at bar may not be factually treated as related to any
class or classes of cases until it is shown identical with them on all,
or even most, points.

Mortality tables, as one of the bases for predicting length of an
individual’s life, are an example. When it has been necessary to
estimate the expected life of a person involved in litigation in some
of the United States in the middle of the twentieth century, the
Carlisle Table, based on death lists from 1779 to 1787 and census
figures for 1780 and 1787 in two parishes in Carlisle, Scotland, has
been put in evidence on the basis that it will be of assistance in
making the prediction.16

16. Cases of this type are collected in Annot., 50 A.L.R.2d 419 (1956).
Immel, Actuarial Tables and Damage Awards, 19 Osso St. L.J. 240 (1958)
describes the leading tables, their sources and ages.
If the future is uncertain, a great deal of the past is uncertain as well (particularly that part involved in contested actions at law). One need only compare "The sun will rise tomorrow,"17 with "The brig Mary Celeste was attacked by pirates,"18 to be reminded that propositions about past facts are "predictions," on existing information, as to what the "truth" will turn out to be when and if more knowledge is available, and that their probabilities can cover the same range as the probabilities of statements about future events.19 We can change the maxim to read, "The past is uncertain, but man must act," and it will suit as well as the earlier form, to show the necessity for acting on the basis of probabilities.

Sir James Fitzjames Stephen put it this way:

The desire to act and the desire to act successfully are ultimate facts in human nature; but we are so constituted that all actions involve belief, and the world is so arranged that successful actions involve true belief.20

He realized that "true" belief was unattainable, and wrote accordingly in section 3 of the Indian Evidence Act, that a fact should be treated as proved "when after considering the matters before it, the court either believes it to exist, or considers its existence so probable that a prudent man ought, under the circumstances of the particular case, to act upon the supposition that it exists."

Sir James included in "acting successfully" not only making as few mistakes as possible, but also producing decisions acceptable to the community; and his higher evaluation of the latter led him to say in effect that more mistakes, rather than less, should be made if they were more acceptable.

He was writing about criminal trials. The extent to which the probability that a fact existed must exceed the probability that it did not, before a reasonable man will act as if it existed, does not depend solely on what the estimate is, or how it was arrived at. The consequence of an error; that is to say, the "value of the probability," is involved. If Gibbon's lottery of 1000 penny tickets is organized to choose one victim for public hanging, the holders of the other 999

17. The probability of which is largely based on the fact it has risen close to $10^6$ times during written history. La Place's theorem would on this basis give it as $\frac{10^6 + 1}{10^6 + 2}$ (assuming a uniform distribution of the possible initial probabilities). We have other reasons which would raise it (based on how other things have behaved).
18. A minor uncertainty may be whether the name is Mary or Marie, but we should put this statement's probability much closer to ½ than the other's. BALDWIN, SEA FIGHTS AND SHIPWRECKS 243 (Dolphin ed. 1958).
19. It was the great insight of Ames that sense-perception itself is a form of prediction for action purposes. THE MORNING NOTES OF ADELBERT AMES, JR. (Cantril ed. 1960). Good starting points are at 7 and 126.
tickets to receive $1,000, you know at once why you do not wish to enter, despite your having the better of the odds themselves. In criminal trials, the requirement that the jurors must find the probability of guilt very high before convicting is not a protection against mistakes. On the contrary, unless their estimates be treated as worthless, or the mistakes more on one side, it insures that the proportion of mistakes will be larger than it would be if every inequality of probability were a guide to decision. But by increasing the total of mistakes, it is supposed to decrease not only the proportion but the absolute number of one kind of mistake—conviction of the innocent. The degree of inequality required reflects an evaluation of the utility of convicting fewer innocent men against the cost of acquitting more guilty ones. This statement of the rule and the reason will serve for the many which could be cited:

In civil cases, where the mischief of an erroneous conclusion is not deemed remediless, it is not necessary that the minds of the jurors be freed from all doubt; it is their duty to decide in favor of the party on whose side the weight of the evidence preponderates, and according to the reasonable probability of truth. But in criminal cases, because of the more serious and irreparable nature of the consequences of a wrong decision, the jurors are required to be satisfied beyond any reasonable doubt of the guilt of the accused, or it is their duty to acquit him; the charge not being proved by that higher degree of evidence which the law demands.

For present purposes the point is that probability theory is being applied without any qualms, with concomitant assumptions—such as that the jury’s errors will be distributed so that this upward movement of the standard in criminal cases will accomplish the purpose. It would be rewarding to know how far the standard has been moved, and what effect this has had upon the proportion and number of the two kinds of mistakes, but it is clear that precise information has not been considered a prerequisite to the action.

When we come to the requirement of proof in civil actions, the case stands different. The problem then is whether there is a basis for giving a higher value to one of the two kinds of mistake, and applying a requirement which is intended to (1) increase the total number of mistakes, and (2) change the proportion and number of one kind of mistake. The majority of courts seem to have said

21. See Good § 7.4, at 83, discussing these “errors of the first and second kinds.”
22. The reader who is disturbed by “cost” and “utility” in this connection may prefer: “Tutius semper est errare in acquietando quam in puniendo, ex parte misericordiae quam ex parte justitiae.” 2 Hale, Pleas of the Crown 290 (1847).
Standards of Proof

There is 'not. Although the reasons cannot be said to be well articulated, they seem to me to be about as follows. In ordinary actions, the law ignores all the costs and utilities which might be consequences of the judgment except the benefit and loss represented by the sum of money or the property awarded or refused. This means that the cost or value of the decision is the same to each party, and the standard should therefore be the one which causes the smallest number of mistakes. It is assumed in these courts that the number of mistakes will be as small as it is possible to make them at the time a decision has to be made, if the jury's estimate is accepted and acted upon whenever it asserts an inequality between the probability that the subsumed facts happened and the probability that they did not happen. This assumes that the jury's mistakes are distributed about the "true" estimate in such a way that their number will be at a minimum if the jury's estimate is always used, since if we knew that juries had a consistent error on one side\textsuperscript{24} we could decrease the total mistakes by changing the standard to allow for it. Those cases in which the jury finds the probabilities equal are disposed of by directing them to decide the issues against the party who has the risk of non-persuasion. If the jury is exactly right, one way of looking at this is to say that this class of cases is one by which the number of mistakes of one kind only is necessarily increased although the total of mistakes is not. It might be thought that this decreases the number of mistakes, on an argument which runs this way. The risk of non-persuasion is allocated (a great part of the time, at least) upon the basis of the probability of the existence of the fact in the run of cases of the particular kind, absent any specific evidence.\textsuperscript{25} Since the evidence and the jury's consideration of it have come to naught, we will make the fewest mistakes if we let the case fall back into the general class, to be decided on those original probabilities. But the jury, unless it lacks the common knowledge we ascribe to it by definition, has begun its own deliberation with those probabilities in mind, and it is the combination of both those and the probabilities drawn from the specific evidence,

\textsuperscript{24} There is some evidence that tendencies toward certain errors may exist. See the examples given in the discussion of the learning model of W. K. Estes, in Kemeny, Snell & Thompson, Introduction to Finite Mathematics 334, 340 (1957); Cohen, Subjective Probability, in Scientific American, Nov. 1957, p. 128. Some of the estimates, even when no changes by the experimenter are expected, seem to resemble those expected from some form of game theory; but the average jury would no doubt be startled at the suggestion that it takes a Manichean rather than an Augustinian view of the universe.

\textsuperscript{25} Stone, Burden of Proof and the Judicial Process, '60 L.Q. Rev. 262, 278 (1944), analyzes the placing of a risk of persuasion, and the result where no evidence is available, on this basis (and in frequency-distribution terms).
that the jury says are at a balance. If we then use the initial probabilities to remove the balance, we are in some sense counting them twice. These balanced cases, if the jury is correct, should go half of them one way and half the other, on a finer classification. We make the same total number of mistakes if we throw them half one way and half the other by lot, or throw them all either way. But by throwing them all one way, all the mistakes made are of one kind. Short of new trials, or the lot system, there seems no way of changing this situation, but the number of cases in it can be minimized.

Our discussion so far clearly points, for civil cases, to a measure of persuasion based on any inequality perceived by the jury between two inversely variable probabilities; the probability that the event required to be found did occur, and the probability that it did not. The majority of courts and writers seem to agree with this view; but two questions, related to each other, arise. Must these probabilities be "found by the preponderance of the evidence" alone? And is there an added requirement, a feeling of "belief," which can exist contrary to the preponderance of the evidence and which must concur with it before a finding can be made?

A few examples of statements based upon some form of "belief theory" of probability will indicate where the difficulties lie:

It has been held not enough that mathematically the chances somewhat favor a proposition to be proved; for example, the fact that colored automobiles made in the current year outnumber black ones would not warrant a finding that an undescribed automobile of the current year is colored and not black, nor would the fact that only a minority of men die of cancer warrant a finding that a particular man did not die of cancer. [citing cases] The weight or ponderance of the evidence is its power to convince the tribunal which has the determination of the fact, of the actual truth of the proposition to be proved. After the evidence has been weighed, that proposition is proved by a preponderance of the evidence if it is made to appear more likely or probable in the sense that actual belief in its truth, derived from the evidence, exists in the mind or minds of the tribunal notwithstanding any doubts that may still linger there. . . .

Upon the evidence . . . a jury could find, not merely that there was a greater chance that the insured met his death by accident falling within the policy than that he met a different fate, but that death by accident within the policy was in fact indicated by the preponderance of the evidence.26

We could call this a "belief in the truth" theory.

No prudent man would act as to a matter of importance to him if, after talking with several of his acquaintances, his state of mind is:

"I think I will make this investment since what I have heard favorable to it impresses me more than what I have heard against it." People of prudence do not take important action involving their self interests when they know no more than that the evidence for taking an important step is stronger for taking it than it is for not taking it. Before acting they entertain stronger convictions.27

This writer, as his context shows, urges a "preponderance of probability" theory, but this example suggests that this probability is not the same as the probability shown by the evidence, and the words "stronger convictions" could mean some extra margin.

A sues B on a note, whose execution B denies. Six witnesses affirm that the signature is in B's handwriting. Five affirm that it is not. No difference in competence, or trustworthiness, between these witnesses appears. Six, however, are more than five. The ordinary man, juror or judge, would say that the evidence "preponderated" in favor of A's proposition. But would the ordinary discreet man believe that proposition? Instead of six let us suppose twenty witnesses, and instead of five let us suppose nineteen. Still there is a preponderance towards A's contention. But would a sensible man necessarily believe that B signed the note, when nineteen men, each equally credible with each of the twenty, said that he did not sign it? In such a state of the evidence, the prudent and careful man would remain in a state of doubt. He would say, "There is one-nineteenth more evidence in favor of B's having signed than in favor of his not having signed, but I am not convinced that he signed it, I neither believe nor disbelieve that he signed it." . . .

There can be evidence that fact X occurred, when it did not occur, and evidence that fact X did not occur when it did occur, and for the same reason, there can be more evidence that it occurred than that it did not occur, although it did not in fact occur, and to believe that there is this greater degree of evidence of occurrence than of non-occurrence, is not to believe the occurrence rather than the non-occurrence.28

If we return to probability theory, we may get a new view of the matter. An example of Poincaré's, in which the estimate of one of the two probabilities involved is such as most people would accept, is as follows:

An effect may be due to cause A or cause B. The effect is observed.

28. Trickett, Preponderance of Evidence and Reasonable Doubt, 10 The Forum 75, 77 (1966). This passage is quoted with seeming approval in 9 Winmore, Evidence § 2498 (3d ed. 1940), and has found its way into the opinions, Bornstein v. Metropolitan Bottling Co., 26 N.J. 263, 139 A.2d 404 (1958). See James, Burdens of Proof, 47 Va. L. Rev. 51, 53 (1961), quoting it approvingly and adding: "All would agree that what counts is the jury's belief in the existence (or non-existence) of the disputed fact, and the extent to which the evidence actually produces that belief; surely we are not seeking the jury's estimate of the weight of the evidence in the abstract, apart from its power actually to convince or persuade them. Moreover it is doubtful whether such abstract weighing can be done (except quantitatively as by counting noses)."
We ask the probability that it is due to cause A. This is a \textit{posteriori} probability. But I could not calculate it if a convention more or less justified did not tell me in advance what was the \textit{a priori} probability for cause A to come into play; I mean the probability of this event for some one who had not observed the effect. Take ecarté—my adversary deals for the first time and turns up a king. What are the odds he is a sharper. The ordinary rules give \(8/9\), a result evidently surprising. But if we look, we see that the calculation is made as if before sitting down I had considered his honesty as only an even chance. This is unjustified here, and is why the result surprises.\(^2\)

The probability estimate drawn from the "effect" of turning a king from the deck is made by assuming that honesty would mean a "thorough shuffle," from which each card has about an equal chance of being the one turned up; that a "sharper" means a person who can and will turn up a king every time he deals; and from the knowledge that the usual ecarté deck contains 4 kings and 28 other cards and that the dealer gets a bonus if but only if the card he turns to five trump is a king. We then compute the probability that a king does come up absent cheating as being \(4/32=1/8\). Poincaré's "ordinary rules" proceed on the surface as follows: We assign the probability 1 to the dealer's getting a king if he cheats; the probability 0 to his failing to get a king if he cheats.\(^3\) We know he turned a king, and it is the ratio of the probability that he did this by cheating to the probability of the total cases of kings, both with cheating and without, that we wish to know. Since he turned a king, the only probabilities involved are those of turning a king by cheating and by chance. We then have \(\frac{1}{1+1/8}=8/9\) as the probability that he did it by cheating.

Poincaré's warning points out that this is not the whole story. Although the explanation seemed to use only the probabilities of getting a king with cheating and without, it contained an assumption (or convention) that there was an even chance that the dealer would cheat, \textit{before} we saw him turn up the king. In other words, it is not enough to judge the probability of a king if cheating occurs and the probability of a king if it does not. Knowing the probability that cause \(A\), if present, produced the effect, and the probability that cause \(B\) if present produced the effect, is not enough. We must know or assume the probability that cause \(A\) would come

\(^{29}\). \textit{The Foundation of Science} 155 (1923).

\(^{30}\). Many probability writers would not allow an empirical probability to be set at 1 or 0; we cannot be absolutely certain. If, however, we were to use a number representing "almost certain," say \(\frac{10^4}{10^4+1}\), instead of 1, and \(\frac{1}{10^4+1}\) instead of zero, our results would change only at the fifth decimal place.
into play and the probability that cause B would come into play at the start. To a person who had not yet learned about the king, and who thought the dealer as likely to cheat as not, the probabilities would be:

- of cheating and turning a king: $\frac{1}{2} \times 1 = \frac{1}{2}$
- of cheating and turning another card: $\frac{1}{2} \times 0 = 0$
- of not cheating and turning a king: $\frac{1}{2} \times \frac{1}{8} = \frac{1}{16}$
- of not cheating and turning another card: $\frac{1}{2} \times \frac{7}{8} = \frac{7}{16}$

On seeing the king, this observer drops the cases involving no king, and has $\frac{1}{2} \times \frac{1}{16} = \frac{8}{9}$ as his estimate based on his original estimate combined with the additional knowledge he has gained, of the probability that the dealer is a sharper. It will not do to say that in the first case we made no assumption about the likelihood of cheating at the start, because our final estimate is exactly what it is with that assumption, and any other assumption will change the final estimate. If the observer, before the game starts, thinks the probability that the dealer is a sharper is $\frac{1}{100}$, the reader can see that after receiving the “evidence” of the king, his final estimate of the probability that the dealer is a sharper will be

$$\frac{1/100}{1/100 + (99/100 \times 1/8)} = \frac{8}{107.31}.$$  

Instead of going from .50 to .89, the probability goes from .01 to .07. And even this is too high; for many people, consciously or not, would set the original probability much lower, as shown by the amount of evidence they would require before judging that the final probability was odds-on. Many would require five kings in as many deals, showing that their original estimate of the initial probability that “this dealer is a sharper” is in the range of $\frac{1}{4,097}$ to $\frac{1}{32,767}$ using an extension of Poincaré’s “ordinary rules.” It is not contended that the initial probabilities used for purposes of illustration are the “right” ones. The examples could have been worked out using a pair of limits throughout, or a set of inequalities. The point is that the probabilities obtained when information is added depend on the initial probabilities as well as on the probabilities drawn from within the new information.

The parallel with a judicial trial is clear. If, in Poincaré’s example, we had been told that only the turning up of the king was evidence, and exhorted to decide solely upon the basis of the evidence, and to

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31. KEMENY, SNELL & THOMPSON, AN INTRODUCTION TO FINITE MATHEMATICS 136 (1957) is one place in which this form of Bayes' Theorem can be found. GOOD § 6.1, gives a slightly different technique which would produce the same final probability.

32. If they place their odds in this range, then regardless of method, they will agree that the probability has not passed .50 after the fourth king, but has passed it after the fifth.
decide upon the “preponderance of the evidence,” we might have concluded that these instructions required an answer which was at variance, not only with our body of judgments on the subject but with any ordinary methods of applying them. If in addition we were told to weigh the evidence produced “on one side,” against that produced “on the other side” and the evidence of the king had been produced by the party asserting that dealer was a sharper, we should be in even worse case. It is ironic that the jurists who deny the applicability of any calculus of probabilities to legal proceedings on the ground that information about the probabilities is imprecise should make us throw away the information we have because it is not “in evidence” and then force us to apply the principle of assigning equiprobability to alternatives on the basis of equal distribution of ignorance.

If we examine the first of the statements set out as illustrations, that of Lummus, J., we find that on the surface it overstates what I have urged as the correct standard. The rhetorical reference to “chances,” with a view to suggesting that to act on anything less than “the truth” is gambling, we can pass by. If an extra margin of inequality is required, such as 60 per cent probability, it seems completely unjustified. Since it is specified as a feeling of the individual juror, called “belief in the truth,” it is clear that each juror is free to consult himself to see whether it exists, and is therefore free to set the margin anywhere from more than “somewhat” above 50 per cent to 99.9 per cent, or higher. If a juror announced on voir dire that he would not believe in the truth of any fact unless it was beyond all doubt, he would presumably be excused unless he proved amenable to “reason”; but in the absence of such inquiry he seems free to raise the standard anywhere, from half-mast to the top of the pole. And the words “convince,” and “actual truth,” and the reference to lingering doubts, suggest a high position. The court’s examples are more puzzling, because no limit to the inequality is actually specified, except the word “somewhat.” As has been shown, we know no more about the “actual truth” in the example, if the figures are 5,000,000 colored cars and 2 black ones—all we know is how to make 5,000,000 (less 2) correct decisions as against 2 errors, in cases of this class. If it is 2,600,000 to 2,400,000, we know the same amount about the actual truth, and have the means of making either 48 per cent of mistakes or 52. No reason is given why we should prefer 52, except that the findings leading to 48 per cent would not be “warranted.” To my mind, the Massachusetts court, and others which have given similar illustrations of their requirements, have, perhaps without intention, given consideration
to an idea wholly outside their explanations. If we picture a litigant
who has the burden of proving that a car was colored rather than
black, in the courtroom with the figures of all manufacturers as his
sole evidence, in an unspecified law-suit, we mentally supply all the
lawsuits we can readily think of as the possible setting. In most
of them, we feel, it would easily be possible for the plaintiff to
produce more specific evidence about the color of the car, and we
draw the inference, from the fact that he has withheld it, that it
shows the car was black. That, such evidence might also have been
produced by opponent is overbalanced by the notion that proponent
has “more access” to such evidence, and we assign an initial proba-
bility which equals or outweighs that drawn from the evidence
alone. If this be the explanation, it is a type of inference which the
courts have always recognized, although the “more access” judgment
is dubious in the bald example. If, however, we are to allow for
such possibilities by raising the measure of persuasion in lawsuits
generally, we have a cure out of all proportion to the malady. In
result, the Massachusetts court seems not to raise the standard as
high as it says it does, as a look at the evidence on which the finding
of accidental death was affirmed will show.

In the second statement, by Professor McBaine, we have something
which may seem to be of the same kind as in the first. McBaine
himself advocates the preponderance of probability as the test in
civil cases, but contends that “degree of belief” is the only feasible
measure of probability. This does not free the juryman to raise his
sights, but the example speaks of being more “impressed” by the
evidence for than that against, and yet not deciding in accord with
this feeling. The question is whether there is being inserted here an
initial probability that restores the balance. It would have to be
some such proposition as “The probability that investments of this
kind lead to losses is so great as to equal or outweigh the probability
produced by the evidence alone.” If this were the case, it would, I
think, have been specified. I believe the correct explanation is a
lapse from the view, already explained, that in civil cases, the
value of the decision should be treated as equal on each side, so
that any inequality of final probability would carry the finding.
Instead of a lawsuit, the example is a proposed investment, and it
seems to me that the alternative to making it that we are allowed
to assume is that if it is not made, the money will repose in high-
probability safety at a comfortable, although lesser, rate of return.
The investment, although slightly more likely than not to succeed,
will cost the principal if it fails. With these inequalities in the value
of the chance in mind, we should no more act than Professor Mc-
Baine. We should await, not “stronger convictions,” but a better investment, considering both the opposing probabilities and the cost and value of the consequences. This same overlooking of the value of the consequences of each kind of error, and mistakenly assuming in an ordinary civil suit that the decision is between spending and saving, lies, I think, behind a great many of the decisions which demand some specified margin of probability before a finding is made. Once it is understood, as it seems to me it should be, that it is as costly to refuse to shift a loss by mistake as it is to shift one by mistake, and as costly to decide in error that there has been no loss as to decide in error that there has been one, we are at once thrown back to every perceptible inequality as the test; realizing that all the mistakes made when there is no inequality will be made against the party having the burden of persuasion, whereas the rest will be about evenly divided if they are distributed in the way most errors in estimates of continuous variables are in other activities.

The third illustration, a statement by Professor Trickett, presents more problems than the others. It has been quoted by Wigmore, and therefore by some courts, with apparent approval. By stressing belief, it opens up the possibility of differing individual standards with different jurors, which are not the same as differing estimates of what the probabilities are. In this quarter, I have already indicated what I believe to be its error. Apart from that, it is obviously intended to be an example with which the reader will at once agree in result, if he is a sensible, a prudent, man. Here there is no departure from the principle of treating the value of the decision as equal for each party. Either A or B will be out X dollars, and the only question is who ought to be. Trickett says flatly, “If B signed the note he ought to be compelled to pay it.” If not, not. Thus much for a clear statement of the substantive law. And, it would be “in-admissible to hold that absolute certainty of the jury that he signed it,” is required. But there must be belief, even if in a low degree, and “perception of the preponderance of the evidence is quite consistent with want of belief.”

We have seen from Poincaré how this can be, if the initial probabilities are combined with those produced by the evidence. Is there an initial probability that B did not sign? Trickett makes no mention of the setting—we have A, who wants to recover on a note, and B, who alleges it to be a forgery. If we compare some crude estimate of the proportion of notes that are forgeries, with the proportion of men who do not pay notes even when they are not forgeries, would every sensible man say the initial probability is that this note is a
forgery, forgeries being so much more prevalent than the failure to pay notes until sued? Some states have balanced these probabilities, and have produced rules providing that unless the execution of a note attached to plaintiff's petition is denied in a special way, it shall be taken as admitted. One theory behind this is to spare plaintiffs the cost of proof when no real dispute exists. But the rule ought to be based on the decision that a high proportion of notes are not forgeries, else there would be no reason not to extend it to facts in general. In such case, A would seem to have none the worse of that evaluation. Another reason for the pleading requirement may be that B will almost certainly know (and have extrinsic evidence) whether he signed, whereas the creditor frequently has nothing but the instrument. If we assume B has pleaded in whatever way is needed, we have a case in which the defendant, who with logical certainty was a witness to the alleged non-execution, has produced five other persons, so far as appears, to testify that this is not his signature, but has not testified himself. If we give the usual weight to that circumstance, the initial probability strongly favors A's claim. It seems likely that this was not considered in the construction of the example, and that B was omitted because his interest would have prevented him from being as credible as the outsiders. From the text of the article, it seems that we are supposed to treat the initial probability as in balance, and that it is the evidence (the testimony) described that is so even as to be unable to upset the equilibrium.

This is the most difficult problem of all; the problem of determining precisely how it is that Trickett finds the evidence so close to a balance that all sensible, prudent, discreet and careful men would treat it as perceptible but unbelievable. Fortunately, we have for this class of case a little experimental data to go upon. Professor Inbau has conducted an experiment upon the ability of witnesses to recognize handwriting with which they claimed to be familiar. Five types of witnesses were used: (1) law professors who claimed familiarity with their colleagues' signatures; (2) secretaries who claimed familiarity with the professor's signatures; (3) laymen who operated upon a comparison of genuine with disputed signatures; (4) bank employees who did the same; (5) handwriting experts who made expert comparisons. The witnesses in the Trickett problem did not testify directly to execution, but only to whether the signature on the note was in B's handwriting. Under the familiar rule that laymen's comparisons add nothing to what the jury can do for itself, and putting aside the bank employees as in-betweens, the

33. See, e.g., 12 MINN. L. REV. 85 (1928).
classes useful for our purposes are (1), (2) and (5) above. The
witnesses for A and B would be of that genus, and probably similar
to the group composed of (1) and (2). For details, the account of the
experiment should be consulted; for our purposes it is enough to say
that in classes one and two, a total of twelve witnesses were shown
sets of cards bearing four signatures, each purporting to be that of
a person whose handwriting they claimed to know, and were asked
to designate them as genuine or forged. Altogether, the witnesses
made 95 examinations and 380 identifications. The individual wit-
ness’ percentage of identifications made correctly ranged from 54
to 78, with an average of $63.5\%$ Three experts ranged from 93 to
100 per cent on 28 signatures, for an average of 96 per cent. Treat-
ing this as a crude measure of credibility in this type of identification,
we may turn back to the illustration in an effort to weigh the
evidence.

It is given that the signature on the note is either B’s or a forgery,
and that six witnesses say it is B’s hand, while five say it is not. They
are equal in “competence” and “trustworthiness.” If we apply the
ideas of a frequency theory of probability already expressed, we can
take this to mean that they are correct the same proportion of times
in identifying a handwriting with which they are familiar, in this
case B’s. In the example, if we assume them right only half the
time, we have already decided the matter—even if all were on one
side, the probability that the hand is or is not B’s would not be
changed by their testifying. This can scarcely be what is meant by
equally trustworthy. If we use the Inbau experiment as a guide, we
can set each one’s proportion of correct identifications at 63 per
cent. If only a single witness had testified, for A, we should say
that the probability that the signature is B’s is 63 per cent, if we
treat the initial probability as 50 per cent, and apply the Poincaré
example. When we have six testifying this way, and five the opposite,
our task becomes one of combining these collectives, or groups of
frequencies, in such manner as to utilize all of them in the production
of a new frequency distribution—the proportion of times the sig-
nature is genuine given this evidence; or the probability of genuinen-
ess, given the evidence (which includes our estimate of the
credibility of the witnesses). The initial probability will be ignored,
since being taken as .50, it has no effect on the final probability.
We decide to treat forgery and genuineness as mutually exclusive,
\(i.e.,\) the event can have happened in one, but only one, of the two
ways. How shall we proceed?

35. The difficulty of the task can be gathered from the reproductions of
the signature specimens in Hilton, The Detection of Forgery, 30 J. Crim. L.,
C & P.S. 568 (1940).
George Boole, one of the fathers of symbolic logic, suggested a method over a century ago, which we can adapt to this problem. The probabilities that each witness is correct and that he is in error are opposing and their sum cannot exceed unity, which represents all his assertions of this class. If we call the probability that he is correct \( P \), the probability that he is mistaken is \( 1-P \). If we take all the occasions on which these witnesses identify handwritings with accuracy \( P \), we should not find them agreeing all the time. If we assume (or judge) that they act independently, (an assumption which Boole seemed not to realize he was making) the probability that six witnesses will agree and be correct is the product of the separate probabilities that each will be correct. In our case this is \( P^6 \). The probability that they will agree in error is \( (1-P)^6 \). The same rule applies to the five witnesses who testify for \( B \); the probability that they will agree and be correct is \( P^5 \), and that they will agree in error is \( (1-P)^5 \). All the other proportions in which they can agree or disagree, and be right or wrong, drop out, since these two agreements are all we need to compare. The probability that 6 witnesses will agree one way, and 5 the other, including both the case where the 6 are correct and the case where the 5 are correct, is given by \( P^6 (1-P)^5 + P^5 (1-P)^6 \). The probability that the 6 will agree and be right and the 5 agree and be wrong is \( P^6 (1-P)^5 \). The probability for this to be the case out of all the possible cases we wish to consider is thus: 

\[
\frac{P^6 (1-P)^5}{P^6 (1-P)^5 + P^5 (1-P)^6}
\]

If we substitute our 63 per cent estimate for \( P \), we have 

\[
\frac{(0.63)^6 (0.37)^5}{(0.63)^6 (0.37)^5 + (0.63)^5 (0.37)^6} = 0.63
\]

The six witnesses will be right, in other words, 63 per cent of the time, and this would seem to be enough of a preponderance of probability to satisfy even an exacting court, since it is as high as the probability would be if \( A \) had produced one witness and \( B \) none. If we look at the formula we can see that for this special case, it reduces to \( \frac{P}{P + (1-P)} = P \). The powers, which represent the number of witnesses, have dropped out, and we can say now that if \( A \) produces 20 such witnesses and \( B \) 19, the probability that the signature is \( B \)'s is 63 per cent. The case is no closer than it was before, and it will be no closer a case if \( A \) produces 100 witnesses and \( B \) produces 99. One way of thinking of this which may make the result intuitively more satisfying is to say that each pair of opposing witnesses cancels, leaving the balance to be determined, not by the fact that \( A \) has one witness and \( B \) none, but by the credibility of that one witness who remains effective to change the balance from
which we started. If the witnesses were all experts, with a credibility of 96 per cent, the probability that it is B's signature would be 96 per cent.

If we examine the bare formula, we see that the case can become close only as the credibility assigned to the witnesses is lower. If we place it at .50, they are for this purpose, no witnesses at all, and we have a perfect balance. If we place it anywhere above .50, then by so much it is more probable than not that B signed, and the cost and value on each side being equal, we should act on this, our best estimate. We need not assert that this procedure is the only reasonable one, to say that it is reasonable enough to prevent the assumptions and assertions of Trickett and of the courts which take Trickett's view, from being descriptive of the only way a sensible, prudent, discreet, and careful man would look at the matter.

My assumption that the witnesses' credibilities are independent, mentioned above, would be one point of attack if I were to claim that the formula is the only reasonable solution. Boole's formulation was attacked because he overlooked the assumption. An analysis of the Inbau experiment seems to indicate that the assumption of independence may be more justified than not, although the data is not classified for easy testing. No knowledge is entirely independent of all other, so that almost independent is what we mean. In the case of witnesses, lack of independence might mean (a priori) a tendency of persons to abandon their own views in order to agree with others rather than to disagree. If we put the most extreme case, and assume that one witness on each side determines all, and that the rest are sheep, we have a case where there is no preponderance, because each side has in effect one witness, and they cancel each other if their credibilities are equal. This would of course mean that no issue could ever be decided in favor of the party having the burden of persuasion where the case turned on one fact to which each side had at least one witness, unless we could identify the "leaders" and find an inequality in their credibilities, or unless

37. Although such an assumption is based on some notion of psychological independence, as opposed to being suggestible, this is not what is meant by independence in probability theory. If the occurrence of A alone and with B is 60 per cent, the occurrence of B alone and with A is 60 per cent, and the occurrence of both A and B is 36 per cent, all under the same conditions, A and B are independent. If, whatever the cause this relation does not hold, they are not independent. Another way of describing complete independence is to say it requires that \( \frac{AB}{AB} = \frac{A}{A} \).

38. Instead of the 5/11 and 19/39 suggested by Trickett, "Counting noses" seems to me to have little to do with the matter, except to find out how many witnesses there are.
other probability inequalities were involved. If the witnesses be
treated in groups, and as tending to form some coalition based on
the views of a majority, it seems that we should expect the majority
to be, in general, those witnesses who are right, and we are back
to the original situation. Neither Trickett nor the courts make men-
tion of any judgment about independence.

Which, of all various views and procedures, will produce the
fewest mistakes in fact finding? This is a question of fact which
only investigation and experiment could really decide. It is unfor-
tunate to have to agree with Cleary:

In science a theory possesses a recognized provisional and tool-like
character. If the empirical data collected do not support the theory, the
theory is discarded. Since the law never collects any data, it is spared
the embarrassment of having ever to discard a theory on that basis.39

From the standpoint of communication, the same is true. It might
be (and I feel should be) concluded that the instructions to juries
should be revised in such manner as to allow (or even cause) them
to utilize something akin to the processes mentioned here. Our
educational system seems to me to be changing in this direction so
rapidly that insistence upon such utilization, coming from outside
the law, if not from within, can be predicted.40 Instructions which
might allow juries to reach conclusions in the way described in this
article are not lacking. It may be their disorganization and archaic
language that cause the difficulty.41 The suggestions of Morgan
and McBaine, already noted, would probably help whenever substi-
tuted for language that points the other way; but instructions which
tell the jury that they shall use what they know in common as
men, should be juxtaposed with the direction to find “from the
evidence.” Clearly, it must be recognized that the jury brings part
of its knowledge (information) with it to the trial; receives more
at the hearing, and in a very real sense creates still more during the
hearing and in the jury room, by combining all this information and
drawing from the combinations new knowledge which was logically
there all the time, but was psychologically unknown to them until
the combination was made and examined.42 This is what they ought

40. A glance at the methods and problems contained in college (and even
high-school) texts will open any mind previously closed to this develop-
ment.
41. It can safely be said that our lack is not a lack of instructions; but
by the time a word is sufficiently encysted to be proof against reversal, it
seems likely to have passed out of the layman’s language. “Moral certainty,”
for example, belongs with “moral phenomena,” a phrase few people use
nowadays.
42. Existing forms could be modified to this end. Illustrations of what is
to do, and instructions should require rather than prevent it.

I say perhaps instructions should be revised in these ways, because to say how they should read requires a communication analysis of what present instructions do, and why. Judge Wanamaker's questionnaire indicated nearly a quarter-century ago that jurors had more trouble with the term "preponderance of evidence" than any of the other terms submitted, but trouble does not present its own solution. I have tried to present a theory of what jurors should and should not be allowed to do, which is itself a subject of dispute. Without further data on what the present instructions accomplish, one must rely on faith, or on the power of vehemently stated ignorance, to conclude with a bang rather than a whimper. To me, what is needed first is a change in the attitude toward juridical truth, a change which channels intuition into the realization of what can be accomplished with the aid of modern statistical tools.

The desire for certainty sometimes causes us to forget the reality of uncertainty; and the analysis here presented may be intuitively rejected because it is focused upon uncertainty. I submit it, notwithstanding, because I feel, with Dr. Johnson: "Sir, to leave things out of a book, merely because people tell you they will not be believed, is meanness."

now used can be seen in Annot., 144 A.L.R. 932 (1943); and see especially Rosenbaum v. State, 35 Ala. 354 (1859), for a statement of the need to use the knowledge.

43. The Jury Project at the University of Chicago Law School is a major effort in this field, and much other work is being done on group decisions.

44. Trial by Jury, 11 U. of Cin. L. Rev. 119, 191 (1937). The jurors may have removed the trouble by at last ignoring the term.